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LEIBNIZ AND PASCAL.[1]*

IN the History of Mathematics it is generally stated that the higher analysis took its rise in the method of indivisibles of Cavalieri (1635).[2] This assertion, at least as far as the invention[3] of the algorithm of the higher analysis is concerned, is erroneous. In what follows it will be shown, by argument founded on the work of the French mathematicians of the seventeenth century and on the manuscripts of Leibniz, that Leibniz was led to his invention of the algorithm of the higher analysis by a study of the writings of Pascal, more than by anything else.[4]

With regard to the manuscripts of Leibniz, the first letters of the correspondence between Leibniz and Tschirnhaus are weighty; they contain the further discussion of their joint labor during the time that they lived together in Paris (September, 1675, to November, 1676);[5] it is well known that it was during this time that Leibniz invented the algorithm of the higher analysis. Among these letters, one from Leibniz, not hitherto published, which closes the first part of the correspondence between Leibniz and Tschirnhaus, contains a very detailed statement of the studies of Leibniz during his sojourn in Paris; it is beyond dispute of the utmost importance, since it was written only four years afterward and recalls particulars in a most vivid manner.[6]

Next, we have to consider the works of the French mathematicians about the middle of the seventeenth century, especially those of Pascal. We know from the facts of Pascal's life that his father, when he moved to Paris in 1631, joined a circle of mathematicians and physicists,[7] of which the history of science has preserved the names of Mersenne, Roberval, Gassendi, Desargues, de Carcavi, Beaugrand, des Billettes, and others. These were in communication, chiefly through Mersenne, with the mathematicians who did not live in Paris, Descartes, Fermat, and de Sluse; so that about the middle of the seventeenth century all that was best (*die Höhe der*) in the science of mathematics was concentrated in Paris.[8] In this circle Pascal moved, hardly yet out of his boyhood, and excited by his eminent talent astonishment and admiration. As an outstanding characteristic of the works of the mathematicians named above there stood forth the endeavor to abandon the method of Cavalieri as lacking every feature of scientific rigor, and to treat the science according to the methods of the Greek mathematicians.[9] Perhaps the ideas of Kepler, in his *Supplementum Stereometriae Archimedeae*,[10] were of influence, when Roberval and Pascal introduced into geometry the ideas of infinity and the infinitely small.[11]

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* For footnotes of both author and translator see *infra,* pp. 550-560.

As for those works of Pascal, which belong to this subject, we must mention in particular the solution of the problems, produced by him in 1658 under the assumed name of Dettonville, on the cycloid. By this, and by the method that he employed, he surpassed all the mathematicians contemporary with him, and he earned for himself the fame of being the greatest geometer of his day.

The investigation of the properties of the cycloid had occupied the attention of the most famous mathematicians of the seventeenth century. It is reported that, earlier than anybody else and indeed before 1599, Galileo had had his attention called to this curve in consequence of his construction of arches for a bridge; he endeavored to find its area in a mechanical way, by weighing a plate of lead of uniform thickness having the shape of a plane bounded by a cycloid; and he found that it was very nearly three times as great as the area of the generating circle. This result he was unable to confirm theoretically. In 1615, Mersenne had his attention called to the cycloid as generated by a rolling wheel; he spent a great deal of time in investigating the nature of the curve, but without success; so that, in 1643, he corresponded with Roberval concerning the difficulties that he had encountered with respect to the curve. Roberval proved, by the help of the method of Cavalieri as improved by himself, that the area of the cycloid is exactly three times that of the generating circle; furthermore, in 1644, he determined the content of the solids formed by the rotation of the cycloid about its base, about its axis, and about the diameter of the generating circle; also he found the centroid of the area of the cycloid. In consequence of a bodily infirmity that robbed him of his rest at night, Pascal, in order to obtain some distraction from his pain, once more took up the investigation of the cycloid after an interval of fourteen years, in the year 1658. His design was to find the area of any chosen segment of the cycloid, the centroid of such a segment, the volumes of the solids described by such a segment by a rotation round either the ordinate or the abscissa, either by a complete, or a half, or a quarter revolution.[12] Inasmuch as the solutions of the problems hitherto investigated had not been done by any general method, but rather by special artificial ways of procedure, the question was that of specially creating a treatment that was applicable in general. Pascal reverted to the method of Archimedes, for determining the quadrature of the parabola by means of the equilibrium of the lever; he generalized the method,[13] by supposing, instead of geometrical figures, unequal weights not merely at the extremities of the lever (which he follows Archimedes in terming *balance*) but also at several different distances from the fulcrum; of these, by means of the Arithmetical Triangle which he had invented,[14] he determined the sum and the center of gravity. On the advice of his friends, Pascal, in June, 1658, under the alias of Dettonville,[15] determined to propose to mathematicians for solution the problems that he had solved. October 1, 1658, was settled as the last day for sending in solutions. Particular cases of the proposed problems were solved by Huygens, de Sluse, and Wren, before the appointed day; but this was not sufficient to meet the requirements of Pascal. At the request of de Carcavi, Pascal made known the abovementioned method for solving such propositions in a long letter, at the beginning of October, 1658,[16] and added thereto three further propositions with respect to the cycloid. In this letter are combined five essays, which prepare the way for the solution of the problems of Pascal.

i. *Traitté des Trilignes et leurs Onglets.*[17]

In this essay, the determination of the content and the centroid of a "triligne" and its "double onglet" is reduced to the sum of the ordinates of the axis or the base in a triligne; also Pascal showed that the determination of the content and the center of gravity of the curved surface of the double onglet could be expressed as the sum of the sines of the axis.[18]

The next essay,

ii. *Propriétés des sommes simples, triangulaires et pyramidales,*

is an appendix to the foregoing. By triangular sum, Pascal meant the sum of a number of magnitudes, each one multiplied in succession by the corresponding number in the natural scale. In the same way, a pyramidal sum denoted the sum of a number of magnitudes, each one in succession multiplied by the corresponding triangular number.[19]

Then comes,

iii. *Traitté des sinus du quart de Cercle.*

In this, Pascal begins by proving the theorem: "The sum of the sines of any arc of a quadrant of a circle is equal to the product of the part of the base, intercepted between the extremities of the outside sines, multiplied by the radius of the circle." By the help of this theorem, he investigated the sum of the sines of a quadrant of a circle, their squares, their cubes, fourth and higher powers,[20] the sum of the rectangles of each sine of the base into its distance from the axis, the triangular and pyramidal sums of the sines of the base, and so on.

The next essay,

iv. *Traitté des sinus et des arcs de Cercle,*

contains the determination of the sum of all the arcs of a circle measured from the vertex of a quadrant to any ordinate of the axis, the sum of their squares, or their cubes, the corresponding triangular and pyramidal sums, the simple and triangular sums of the sectors, the sum of the solids formed from every sector of a quadrant and the distance of its center of gravity from the base, and so on.

v. *Petit Traitté des solides circulaires.*

In this is investigated the position of the center of gravity of such bodies as are formed by the rotation of half a band of a circle about the axis or base, the sum of the fourth powers of the ordinates of the axis, of their cubes, the position of the center of gravity of the semisolid of revotution arising from a rotation about the axis, and so on.

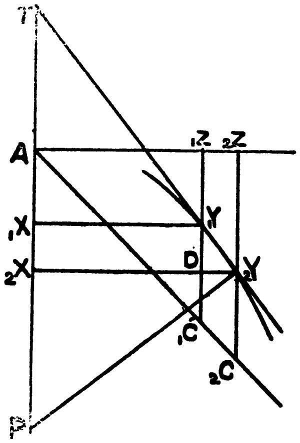
These five essays conclude with:

Un Traitté general de la Roulette, contenant la Solution de tous les Problemes touchant la Roulette qu'il avoit proposez publiquement au mois de Juin 1658.

All these works of Pascal are strictly geometrical in treatment, after the manner of the geometry of the ancients; there is not to be found in them a trace of the method of dealing with geometrical problems introduced by Descartes.[21]

It is well known that Leibniz through his acquaintance with Huygens, who lived in Paris from 1666 to 1681, was encouraged to study higher mathematics. More especially, it was Huygens who advised him to read the writings of Pascal. Upon several occasions later, has Leibniz declared, in conformity with this, that he was led to the higher analysis by the study of the writings of Pascal, and thus made his discoveries; first, in the hitherto unpublished letter to Tschirnhaus, of the year 1679, the part of it that relates to our subject being given later; also in a letter to the Marquis de l'Hospital, in the year 1694; further, in a postscript to a letter to Jacob Bernoulli, in the year 1703; and lastly, in the essay, *Historia et Origo calculi differentialis,* written in the last years of his life.

Up to the present time, among the manuscripts of Leibniz there has been found one of great length, that bears the title: *Ex Dettonvillaeno* (?) *seu Pascalii Geometricis excerpta*: *cum additamentis.* It is not dated; but as it contains work that is in the closest connection with the writings of Pascal to de Carcavi, hence it must be assigned very approximately to the time of his intercourse with Huygens (1673). This cannot be given in its entirety; only the commencement of it follows under the heading III. One special remark has Leibniz made on the five essays which follow Pascal's letter to de Carcavi; he states that the method of Pascal for determining the surface of the sphere,[22] according to which the surface of a solid formed by the rotation round an axis can be reduced to a plane figure proportional to it, was what induced him to make out a general theorem applicable to all plane figures bounded by a curved line.



The coordinates of ${}_1Y$ and ${}_2Y$, two points on the curve, are ${}_1Y_1Z$, ${}_1Y_1X$ and ${}_2Y_2Z$, ${}_2Y_2X$; ${}_2YT$ is the tangent at ${}_2Y$, which is supposed to meet the curve again in ${}_1Y$, and the normal ${}_2YP$ is drawn. On account of the similarity of the triangles ${}_1YD_2Y$and ${}_2Y_2XP$, we have

$${}_2XP\,.\,{}_1YD = {}_2Y_2X\,.\,{}_2YD;$$

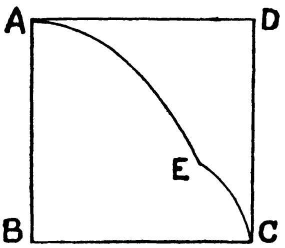
i. e., the subnormal ${}_2XP$ applied, at right angles to the axis AX, to the element of the axis ${}_1X_2X(={}_1YD)$, is equal to the ordinate ${}_2Y_2X$, applied to the element ${}_2YD$.[23] "But," Leibniz continues, "straight lines which increase from nothing, each multiplied by its corresponding element, form a triangle. For, let AZ be always equal to ZC, and you get the right-angled triangle AZC, which is half the square on AZ, and thus the figure produced by applying the subnormals in order at right angles to the axis is always equal to half the square on the ordinate. Hence, being given a figure to be squared, that figure is sought whose subnormals are equal to the ordinates of the given figure, and the second figure is the quadratrix of the given figure. Thus from this very simple idea, we have the reduction of surfaces produced by rotation to plane quadratures, and also of the rectification of curves;[24] and at the same time, we can reduce these quadratures to problems of inverse tangents." Thus it came about that Leibniz obtained from this a general method for the quadrature of curves.

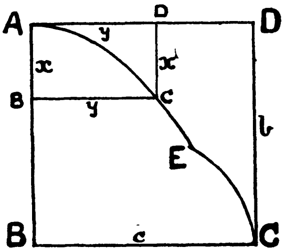
All this was attained to by Leibniz in the first year, 1673/74, of his mathematical studies in regard to the higher analysis. Until this time he had adhered to the rigorous geometrical method, as he found it in the writings of Pascal, in his investigations; acting on the advice of Huygens, he now made himself acquainted with the method of Descartes as being more adapted to computation. The long essay of Leibniz with the title, *Analysis Tetragonistica ex Centrobarycis,* dated Oct. 25, 26, 29, and Nov. 1, 1675, shows clear connection[25] with the above-mentioned method of Pascal; also it shows the improvement that Leibniz had made in consequence of his study of Cartesian geometry, Leibniz commences with Proposition 2 from Pascal's first essay, *Traitté des Trilignes et leurs Onglets,* which he expresses as follows.

"Let any curve AEC be referred to a right angle BAD; let AB $\sqcap$ DC $\sqcap a$,[26] and let the last $x \sqcap b$; also let BC $\sqcap$ AD $\sqcap y$, and let the last $y \sqcap c$. Then it is plain that

$$\text{omn. } \overline{yx \text{ to } x} = \frac{b^2c}{2} - \text{omn. } \overline{\frac{x^2}{2} \text{ to } y}\ .$$

For, the moment of the space ABCEA about AD is made up of rectangles contained by BC $(= y)$ and AB $(= a)$;[26] also the moment about AD of the space ADCEA, the complement of the former, is made up of the sum of the squares on DC halved $(= x^2/2)$; and if this moment is taken away from the whole moment of the rectangle ABCD about AD, i. e., from[26] c into omn.x, or from[26] $b^2c/2$, there will remain the moment of the space ABCEA.



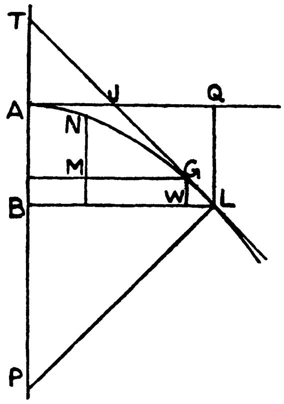


Hence the equation that I gave is obtained; and, by rearranging it, it follows that,

$$\text{omn. } yx \text{ to } x + \text{omn. } x^2/2 \text{ to } y = b^2c/2.$$

In this way we obtain the quadrature of the two joined in one in every case; and this is the fundamental theorem in the center of gravity method."

In the continuation, dated October 29, 1675, in connection with this theorem, Leibniz brings in the characteristic triangle, which has already been mentioned above.



AGL is any curve, $BL = y$, $WL = l$, $BP = p$, $AB = x$, $GW = a$, $y = \text{omn. } l$; hence,

$$\frac{l}{a} = \frac{p}{y} = \frac{p}{\text{omn. } l}\text{, and therefore } p = \frac{\text{omn. } l}{a} . l .$$

Now, by the theorem given above,[27]

$$\text{omn.}\, p = \frac{y^2}{2} = \frac{\overline{\text{omn.}\, l}\ \boxed{2}}{2} = \frac{\overline{\text{omn.}\, l}^2}{2}\ ;$$

hence
$$\frac{\overline{\text{omn.}\, l}^2}{2} = \overline{\text{omn.}\ \overline{\text{omn.}\, l}\ .\frac{l}{a}}\ ;$$

"that is," adds Leibniz, "if all the *l*'s are multiplied by their last, and all the other *l*'s again are multiplied by their last, and so on as often as it can be done, the sum of all these will be equal to half the sum of the squares, of which the sides are the sums of these, or all the *l*'s. This is a very fine theorem, and one that is not at all obvious. So is also the theorem,

$$\text{omn.}\, xl \sqcap x\,.\,\text{omn.}\, l - \text{omn.omn.}\, l,$$

where *l* is supposed to be a term of a progression, and *x* the number which expresses the position or ordinal that corresponds to the *l*, i. e., *x* is the ordinal number and *l* the ordered quantity.

N.B. In these calculations, a law for all things of the same kind may be observed; for, if 'omn.' is prefixed to a number or ratio, or to something indefinitely small,[28] then a line is produced, also if to a line, then a surface, or if to a surface, then a solid; and so on to infinity for higher dimensions.

It will be useful[29] to write $\int$ for 'omn.,' so that

$\int l =$ omn. *l*, or the sum of all the *l*'s.

Thus, $\int \frac{\overline{l^2}}{2} = \int \overline{\int \overline{l}\, \frac{l}{a}}$, and $\int \overline{xl} = x \int \overline{l} - \int \int l$. "

This was the first time that the algorithm for the higher analysis was introduced. In what then follows, Leibniz obtains the first theorems of the integral calculus:

$$\int x = x^2/2,\ \int x^2 = x^3/3,$$

and adds, "All these theorems are true for series in which the differences of the terms bear to the terms themselves a ratio which is less than any assignable quantity."

Further Leibniz remarks: "These things are new and noteworthy, since they lead to a new kind of calculus. Being given l, and its relation to x, required to find $\int l$. Now this may be obtained by a reverse calculation; thus, if $\int l = ya$, suppose that $l = ya/d$, that is to say, as $\int$ increases the dimensions, so d will diminish them; but $\int$ stands for a sum, and d for a difference.[30] From the given value of y, we can always find y/d or l, or the difference for the y's."

In the investigation that bears the title, *Methodi tangentium inversae exempla,* dated November 11, 1675, Leibniz introduces instead of y/d the notation dy.

Such are the chief points in the story of the introduction of the algorithm of the higher analysis, as far as may be gathered from the extant manuscripts of Leibniz.[31]

In connection with the earlier essay, "Leibniz in London,"[32] I have shown that any influence whatever from external sources upon Leibniz with regard to the introduction of the algorithm of the higher analysis is excluded.

KARL IMMANUEL GERHARDT.

TRANSLATIONS OF THE MANUSCRIPTS

Alluded to by Dr. Gerhardt.

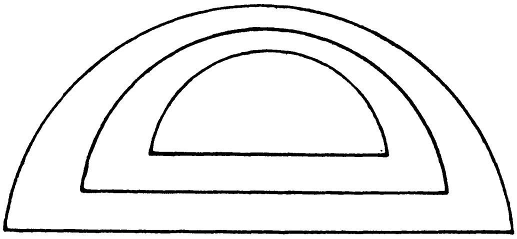
I.

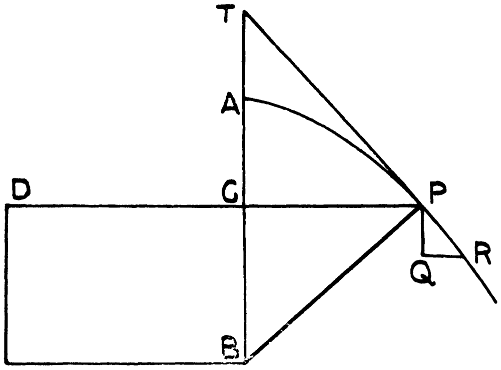
From the letters of Leibniz to Tschirnhaus.

1679.

"You are astonished that Reginaldus[33] should have been able to fall into error over the surface of an elliptic spheroid; but you do not seem to have considered sufficiently how different are the several methods of indivisibles. He certainly understands the Cavalierian method, but that is so circumscribed by narrow limitations that few things of any great importance can be obtained from it. There is no doubt that Cavalieri, Torricelli, Roberval, Fermat, and indeed, as far as I know, all the Italian mathematicians were quite unaware of the utility of tangents for the purpose of finding quadratures, or of that which I have been accustomed to call the infinitely small "characteristic triangle" of the figure; indeed, at the present time also in France, I believe that Huygens is the only man that really understands these matters.[34] Pascal himself could not sufficiently express his admiration for the artifice by which Huygens found the surface of the parabolic conoid. Sluse has given no example of these things, by which I am inclined to think that they are unknown to him also. This too is the reason why Huygens and Gregory demonstrated such theorems by roundabout methods, suppressing their analysis, in order not to divulge their method at once so easy and so fruitful.

"The prime occasion from which arose my discovery of the method of the Characteristic Triangle, and other things of the same sort, happened at a time when I had studied geometry for not more than six months. Huygens, as soon as he had published his book on the pendulum, gave me a copy of it; and at that time I was quite ignorant of Cartesian algebra and also of the method of indivisibles,[35] indeed I did not know the correct definition of the center of gravity. For, when by chance I spoke of it to Huygens, I let him know that I thought that a straight line drawn through the center of gravity always cut a figure into two equal parts; since that clearly happened in the case of a square, or a circle, an ellipse, and other figures that have a center of magnitude, I imagined that it was the same for all other figures. Huygens laughed when he heard this, and told me that nothing was further from the truth. So I, excited by this stimulus, began to apply myself to the study of the more intricate geometry, although as a matter of fact I had not at that time really studied the Elements. But I found in practice that one could get on without a knowledge of the Elements, if only one was master of a few propositions. Huygens, who thought me a better geometer than I was, gave me to read[36] the letters of Pascal, published under the name of Dettonville; and from these I gathered the method of indivisibles and centers of gravity, that is to say the well-known methods of Cavalieri and Guldinus. I immediately committed to paper certain things that occurred to me as I read Pascal, of which I now find that some are absurd, others please me very much even at the present time.[37] Amongst other things, I tried to find a new sort of center. For, I thought that if, to any figure that was given, others that were similar and similarly placed were inscribed, then a "middle point" could be found,[38] at which the figure evanesced, and that being given this point the quadrature could be obtained; later I perceived the difficulty that made this method ineffective. But to return to the subject, I will tell you how I came to find the method of the Characteristic Triangle. Incidentally Pascal gave a proof of the dimension of the spherical surface proved by Archimedes, that is the moment of a circular curve round the axis,[39] and showed that the radius applied to the axis produced this moment. I, having examined the demonstration with care, observed that, with the aid of the infinitely small characteristic triangle, it was possible to prove the following general proposition for any curve:[40]





"Let AP be any curve and let BP be drawn perpendicular to its tangent AT, to meet the axis in B; then, the ordinate PC being drawn, let the straight line CD be applied to the axis AC, perpendicular to it, and equal to BP. Then if a curve is drawn through all such points as D, we shall have a figure whose area will be the moment of the original curve about the axis, i. e., it will show how to draw a circle equal in area to that of the surface of a curve rotated round the axis. Since in the circle the straight line BP is always of the same length wherever the point P is taken in the curve, hence the figure produced by the perpendiculars[41] applied to the axis is a rectangle, and thus the surface of the sphere is very easily reduced to a plane area. Now, when from this method I had deduced a general method for the dimensions of such surfaces, I at once took it to Huygens; he was surprised and laughingly confessed that he had made use of precisely the same method for obtaining the surface of the parabolic conoid of revolution. For in that case the curve through every D is a parabola, and hence the figure is capable of quadrature. Since I wished to verify the accuracy of my result in the case of the parabola,[42] I began to look for a method of expressing spaces and curves by reckoning, and then for the first time I really understood those matters of which Descartes wrote. For, previously, I used to calculate in my own way, using not letters but the names of lines. Then, for the first time, I read Descartes and Schooten carefully, acting on the advice of Huygens, who told me that the method of reckoning adopted by these authors was very convenient. Meanwhile having once opened the door provided by the characteristic triangle, I very easily discovered innumerable theorems with which at that time I filled innumerable sheets; but later I found that these had also been noted by Huraet, Gregory, and Barrow.[43] Moreover all these things I came upon in the first year of my apprenticeship to geometry. But after that I struggled forward to far greater things, such as I believe that neither Gregory nor Barrow could ever have reached by their methods, far less Cavalieri or Fermat.[44] About the same time, since I perceived that the finding of quadratures could be reduced to the finding of sums of series, and that the finding of tangents could be reduced to the finding of differences, I put together the fundamental principles of my new calculus,[45] which I call the "differential or tetragonistic calculus," by which I can set with a few little lines those things which could be obtained with great difficulty, if indeed at all, by the help of a mighty apparatus of lines. Moreover I considered in general that the finding of the sum of any series was nothing else but the discovering of some other series, the differences of the terms of which gave the given series, and this other series I used to call the summatrix.[46] The occasion for considering infinite series arose from the work of Wallis and Mercator.[47]

When I joined their discoveries to mine, I found out new things with no trouble at all.

"At length, when I considered that problems of quadratures might not be of known degree, and yet might be reduced to equations, in which the exponents of the powers were unknowns, a new light dawned upon me and I began to understand that this was something beyond the ordinary analysis, and I called it transcendent, because it employed equations beyond all degrees; and I see that this method, almost alone of its kind, gives a method of determining whether particular problems of this kind are possible or not. Indeed I can easily prove in other ways, and also by the differential calculus more especially, the impossibility of general quadrature of the circle, or that no algebraical line can be given as its quadratrix. What I call algebraical lines are those that Descartes calls geometrical, and by quadratrixes I mean all curves that, being described, will give the quadrature of any portion of a circle whatever. But the manner of finding the impossibility of any particular quadrature, for instance that of the whole circle, is known to me indeed in two ways, the one by the calculus of transcendent exponents, the other by a certain new kind of calculus, embracing all cases, which has not entered the mind of any one before even in his dreams.[48]

"Here you have the story of some of my meditations...."

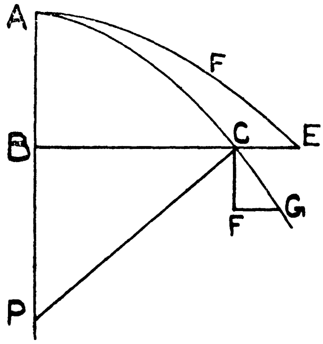
II.

From the correspondence between Leibniz and the Marquis de l'Hospital.

1694.

"I recognize that M. Barrow has advanced considerably, but I can assure you, Sir, that I have derived no assistance for my methods (*pour mes methodes*).[49] At the start I only knew the indivisibles of Cavalieri,[50] and the 'ductions' of Father Gregory St. Vincent, along with the "Synopsis of Geometry" of Father Fabri, and what could be derived from these authors and their like.[51] When M. Huygens lent me the "Letters of Dettonville" (or Pascal), I examined by chance[52] his demonstration of the measurement of the spherical surface, and in it I found an idea that the author had altogether missed; for I remarked that in general, by the same reasoning, the perpendiculars PC, when applied to the axis or set in the position BE, give a line FE, such that the area of the figure

FABEF will furnish a development (*explanation*) of the surface formed by the rotation of AE about AB.



"Huygens was surprised when I told him of this theorem, and confessed to me that it was the very same as he had made use of for the surface of the parabolic conoid. Now, as that made me aware of the use of what I call the "characteristic triangle" CFG, formed from the elements of the coordinates and the curve, I thus found as it were in the twinkling of an eyelid nearly all the theorems that I afterward found in the works of Barrow and Gregory. Up to that time,[53] I was not sufficiently versed in the calculus of Descartes, and as yet did not make use of equations to express the nature of curved lines; but, on the advice of Huygens, I set to work at it, and I was far from sorry that I did so: for it gave me the means almost immediately of finding my differential calculus.[54] This was as follows. I had for some time previously taken a pleasure in finding the sums of series of numbers, and for this I had made use of the well-known theorem, that, in a series decreasing to infinity, the first term is equal to the sum of all the differences. From this I had obtained what I call the "harmonic triangle," as opposed to the "arithmetical triangle" of Pascal; for M. Pascal had shown how one might obtain the sums of the figurate numbers, which arise when finding sums and sums of sums of the natural scale of arithmetical numbers. I on the other hand found that the fractions having figurate numbers for their denominators are the differences and the differences of the differences, etc., of the natural harmonic scale (that is, the fractions 1/1, 1/2, 1/3, 1/4, etc.), and that thus one could give the sums of the series of figurate fractions

$$1/1 + 1/3 + 1/6 + 1/10 + \text{etc.}, \qquad 1/1 + 1/4 + 1/10 + 1/20 + \text{etc.}$$

Recognizing from this the great utility of differences and seeing that by the calculus of M. Descartes the ordinates of the curve could be expressed numerically, I saw that to find quadratures or the sums of the ordinates was the same thing as to find an ordinate (that of the quadratrix),[55] of which the difference is proportional to the given ordinate. I also recognized almost immediately that to find tangents is nothing else but to find differences (*differentier*), and that to find quadratures is nothing else but to find sums, provided that one supposes that the differences are incomparably small. I saw also that of necessity the differential magnitudes could be freed from (*se trouvent hors de*) the fraction and the root-symbol (*vinculum*), and that thus tangents could be found without getting into difficulties over (*se mettre en peine*). irrationals and fractions.[56] And there you have the story of the origin of my method...."

[At this point Gerhardt quotes his article, *Leibniz in London,* and a long passage from the *Historia,* in corroboration of the foregoing letters. I have omitted them as I have already, in my notes, pointed out the points of resemblance, and the slight differences, between the several accounts that Leibniz gives.—J. M. C.]

III.

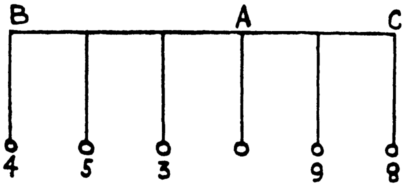
Extracts from the geometry of Dettonville or Pascal; with additions.

Ca. 1673.

$$\begin{array}{cccc} 1 & 2 & 3 & 4 \\ A & B & C & D \\ & B & C & D \\ & & C & D \\ & & & D \end{array}$$

If A, B, C, D, are quantities, their triangular sum, starting with A, is 1A, 2B, 3C, 4D.

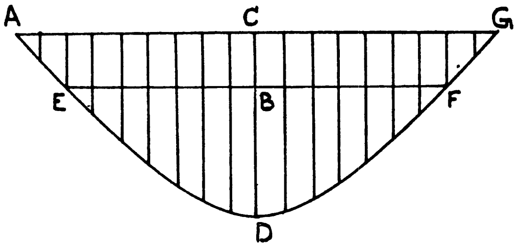
If BC is any straight line divided into any number of equal parts, and any weights, equal or unequal, are suspended at the points of division, and A is supposed to be their point of equilibrium, it is necessary that the triangular sum of the weights on the one arm AB should be equal to the triangular sum of the weights on the other arm AC, where the triangular sum on either side starts from the inner point or from the side A. The reason is that the weights give an effect that is compounded of the ratio of the weights and their distances from the center. But these distances, on account of the division



of the straight line or beam of the balance into equal parts increase as 1, 2, 3, etc.

This is what Pascal says; to which I add the following remarks.

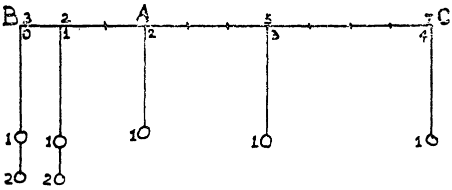
Even if the triangular sums on either side of the point are not the same, that is if the two arms are not in equilibrium, yet the moments will always be to one another as the triangular sums, for the moments are always equal to triangular sums.[57] Hence the far more general rule: If any straight line is divided into any number of equal parts, and weighted with any number of weights suspended at the points of division, and if any point of division is taken to be A, then will the moments of the weights on the one arm BA be to the moments of the weights on the other arm CA as the triangular sums starting from that weight which is nearest to A on each side.[58] Also when any figure, i. e., a line, a surface, or a solid, can be put in such a position that a certain line in it can be taken as parallel to the horizon, that straight line can be taken as a balance, and all the points or all the straight lines or all the planes (where the points in the line are assumed to be placed horizontally, or lying in planes of these points set perpendicular to the horizon), may be considered as weights; and thus, if the quantity or progression of these weights is known, and consequently their triangular sum, then the center of gravity of the figure is known; not indeed its position in the figure, but its position in the straight line that has been taken. The center of equilibrium in the figure itself is of this nature: namely, that a straight line passing through it will cut the figure into two parts, such that on each side the triangular sums of the points, straight lines, or horizontals of the solids are equal to one another. Hence the center of gravity of the whole figure being found, the centers of gravity of arms of this kind supposable without the figure may be obtained; for, let the figure be A, and



let there be taken a line parallel to the horizon in which is the center of gravity B, and suppose that the center of gravity of it is placed above a horizontal style or suspended by a thread: then it is plain that the figure will be in equilibrium. But if it is in equilibrium, then the straight line CD, drawn through the center of gravity, will cut the figure in such a fashion that the triangular sums on each side are equal; and if moreover another straight line perpendicular to CD is supposed to be divided into an infinite number of parts by the infinite parallels to CD, the triangular sums of the infinite rectangles on each side will be equal to one another, for by hypothesis the rectangles can be supposed to be suspended as weights from EF as a balance at the points of division (from which it is clear that the suspended weights need not necessarily be understood to be perpendicular to the horizon, but they may be parallel to it). This being the case, the position of the figure may be changed from the horizontal to the perpendicular, and AG become the balance; in which case it is clear that the point of equilibrium will fall at C, since the triangular sums are by hypothesis equal on each side of it. Hence, given the center of gravity of any figure, and assuming a balance either without or within the figure, to which the figure is supposed to be rigidly attached, the point of equilibrium can be found in it, by merely drawing a perpendicular to it through the center of gravity; for this will cut the balance in the point of equilibrium. On the other hand, if the points of equilibrium of two balances for the same figure are given, the center of gravity for the figure can be found (whether it is within or without the given figure; for sometimes the center will fall within the given figure; and sometimes without, as in the case of annular figures, or curved lines, or other incomplete things); that is to say, at the point of intersection of two perpendiculars drawn from those two balances toward the same parts, in the same plane, if the figure is a plane figure, i. e., if the balances are in one and the same plane; but if the two balances are not in the same plane, there is need for three. This is to be investigated.[59]

But the following is a better way: Suppose that the figure is first affixed to one balance, and let the plane through the common perpendicular be the balance and the horizontal be drawn through the point of equilibrium to cut the figure; then let the figure be affixed to another balance, and once more let another plane be drawn to cut the figure; the intersection of these two planes will give a straight line which will contain the center of equilibrium. If now a third balance is taken in addition, or a third plane, the point of intersection of all the planes, or the point in which the third plane cuts the line already found, will be the center of equilibrium. But if the figures are planes, then two balances and two perpendiculars are sufficient; and also if they are curved lines that lie all in the same plane.

Now it is worth while noting several things in those cases in which the balance is not divided into equal parts; for it may happen that we may know in some way or other the sums of the weights and their progressions, but they are such that, when applied to the balance, they divide it into unequal parts; in that case the progression of the parts into which the balance is divided has to be investigated, as for instance if it is divided into parts that continually increase according to the squares or otherwise. Thus, if we wish to suppose that the weights are equal, while the balance is divided into parts that increase as 1, 2, 3, 4, etc., and yet that this case may come under the rule, we must proceed in this way. Suppose that that point of equilibrium is already found and that it is



2, say; then it is clear that, starting from the point 2 assumed to be the center, the arms should be numbered, and that the point 1 should be marked with the number 2, and the point P with the number 3, and on the other side the point 3 should be marked with the number 3, and the point 4 with the number 7. Now, supposing that the weights are multiplied by the numbers of their own points or arms, it is necessary that the product obtained should be equal;[60] but if it is not, then another point must be sought (or something should be added to, or subtracted from, the weights; for instance, in this case, if the weights are 2, 3 should be supposed to be doubled, or in place of 1,1 we write 2,2 underneath, then there would be an equilibrium on each side, of 10). But to obviate the necessity of going through all the points, a formula should be sought; but if no known progression can be employed for the weights and the parts, a formula will be impossible; but when a known progression can be obtained, then a formula can be found as far as the nature of progression will allow. But the greater part of the difficulty will vanish in those cases in which the weights can be assumed to be equal. What is more, a very simple general rule has been found which is the reciprocal to that of Pascal, namely, that a point may be assumed such that the triangular sums of the numbers on each arm, always starting from the end and going toward the middle, are equal......[61]

NOTES.

A. To Dr. Gerhardt's Article.

[1] [Translated by J. M. Child from Dr. K. I. Gerhardt's article, "Leibniz und Pascal," in the *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin,* 1891 (Zweiter Halbband), pp. 1053-1068. All notes added by the translator are put in square brackets.]

[2] "When I speak of the geometry of indivisibles," says Leibniz, "I intend something far more comprehensive than the geometry of Cavalieri, which does not appear to me to be anything but an insignificant (*mediocris*) part of the geometry of Archimedes." [The general statement appears to me to be nearer the truth than that of Gerhardt, who lays unjustifiable stress on the above remark of Leibniz. I have endeavored to show later that there is strong probability that the work of Cavalieri, which Leibniz in the *Historia* acknowledges to have read (see *The Monist,* Oct., 1916, p. 593; also pp. 583, 585 in the same number), was the *Exercitationes Sex,* and not the *Geometria* that was published ten years earlier; perhaps he read them both.]

[3] [It seems to me that those who claim merely the symbolism of the Calculus as an "invention" of Leibniz are really detractors from his genius. I have endeavored to show in previous articles in *The Monist* that this symbolism, more especially as regards the sign of differentiation, was a gradual *adaptation and development* of ideas already preconceived for finite differences, until Leibniz had obtained a standardized symbolism for the infinitesimal calculus. This, in my opinion, evidences an immensely greater intellect than that necessary for an "invention"; even if we do take the standpoint that he was helped by the work of his immediate predecessors. Perhaps Gerhardt's word *Erfindung* might be better rendered by "construction" instead of "invention" or "discovery."]

[4] [There was absolutely nothing in Pascal to suggest the sign for *differentiation,* and Leibniz might just as easily have obtained his ideas on integration from Galileo or others as from Pascal.]

[5] [According to the generally accepted account, Leibniz was in London at the end of the third week of October, 1676, on his way home, via Amsterdam.]

[6] [A point therefore to be carefully noticed is that the figure given for the characteristic triangle is totally different from that given in the "Bernoulli postscript" (see *The Monist,* Oct., 1916, p. 585); it is also different from the figure used by him in the manuscript dated Oct. 1674, which is undoubtedly derived from the figure used by Pascal in the opening lemma to the *Traitté des Sinus du quart de Cercle* (see *The Monist,* April, 1917, p. 241, and compare with *The Monist,* Oct., 1916, p. 615); it is different from either of the figures used in the manuscripts of Oct. 29, Nov. 11, 1675 (see *The Monist,* April, 1917, pp. 257, 262, 281), the last of these being like Barrow's Differential Triangle, as used by him throughout his theorems on quadratures. Does this point to a new supposition: namely, that Leibniz originally invented a certain characteristic triangle of his own, essentially different in small detail from that of Pascal, Barrow, or any one else; that then he gradually passed from this to that of Pascal, later to Barrow's form; that he found this the most convenient of all; finally, through lack of memory, he ascribes the earliest form to Pascal, instead

of to himself, making an erroneous apperception of the time at which he had *discovered* this early from? The point is referred to in a later note (40).]

[7] It went by the name of "Compagnie"; out of it grew, in 1666, the "Académie des Sciences."

[8] [Gerhardt no doubt here refers to French mathematicians; but the first-mentioned names, of those that lived in Paris, with the exception of Roberval hardly bear comparison with those of the three who did not live there.]

[9] The writings of Roberval and Pascal bearing reference to this have been mentioned in the essay "Leibniz in London." [Unintentionally omitted in the Oct., 1917, *Monist.*]

[10] *Nova Stereometria Doliorum Vinariorum, inprimis Austriaci, figurae omnium aptissimae, et Usus in eo Virgae Cubicae compendiosissimus et plane singularis. Accessit Epitome Stereometriae Archimedeae Supplementum. Lincii an. M DC XV.* See my *Geschichte der Mathematik in Deutschland,* pp. 109ff.

[11] Roberval in a letter to the astronomer Hevelke (Hevelius) in Dantzig, writes: "Concerning analysis, in which I delight, I have far more [theorems]; and no fewer concerning the doctrine of the infinite, which they now call the 'doctrine of indivisibles'...." Published in: *Huygens et Roberval. Documents nouveaux. Par C. Henry*; (Leyden, 1879).

[12] [By ordinate and abscissa, Gerhardt means what Pascal calls the axis and base of the segment. Pascal only considered the whole solid of revolution, and the semi-solid, their volumes, their centers of mass, and the centroids of their surfaces; but those for solids generated by a quarter of a revolution could have been deduced quite easily.]

[13] [Pascal, in effect, obtained the general formula

$$\bar{x} = \Sigma(mx)/\Sigma(x),$$

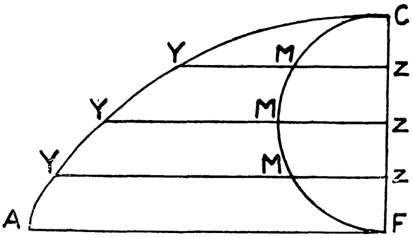
where Σ stands for either a summation of finite quantities, or for the equivalent of integration. If this is to be ascribed to Pascal as an original contribution, then we must assume that he had never seen Cavalieri's *Exercitationes Sex, Exer. quinta,* Theorems 6, 7, 8, and certain others of the fifty propositions that form this section of the book; the section being entirely devoted to centers of gravity, while the method is a direct anticipation of Pascal's.]

[14] [What is generally known as the Arithmetical Triangle is not mentioned in the *Lettres de Dettonville;* see Note 19.]

[15] [It may be of interest to note that the pseudonym of Amos Dettonville is an anagram on Lovis de Montalte; Lovis, or Louis de Montalte being the pseudonym under which Pascal's *Lettres provinciales* appeared.]

[16] Pascal published what he had written to de Carcavi along with the five essays in the following year, under the title of: *Lettres de A. Dettonville contenant quelques unes de ses Inventions de Geometrie. Scavoir, La Resolution de tous les problemes, touchant la Roulette qu'il avoit proposez publiquement au mois de Juin, 1658. L'Egalité entre les Lignes courbes de toutes sortes de Roulettes et des Lignes Elliptiques. L'Egalité entre les Lignes Spirales et Paraboliques, demonstrée à la maniere des Anciens. La Dimension d'un Solide formé par le moyen d'une Spirale autour d'un Cone. La Dimension et le Centre de Gravité des Triangles Cylindriques. La Dimension et le Centre de Gravité de l'Escalier. Un Traitté des Trilignes et leurs Onglets. Un Traitté des Sinus et des Arcs de Cercle. Un Traitté des Solides Circulaires. A Paris, M DC LIX.* This writing contains the essays of Pascal of the year 1658 together with communications to Huygens, de Sluse, and an unnamed correspondent. From the correspondence of Huygens in the years 1658 and 1659, which is printed in that truly great work: *Oeuvres Complètes de Christiaan Huygens publiées de la Société Hollandaise des Sciences,* we see that a great movement arose among contemporary mathematicians through Pascal's problems, as well as through the printed works that we have mentioned. Leibniz expresses himself thus: "By this time, the controversy [referring to Gregory St. Vincent] had cooled down; when lo! fresh movements in the realm of geometry are stirred up through the whole of France, by Blaise Pascal, a man

of the highest genius, and one who at that time had come nearer to the reputation of Galileo and Descartes than any one else."—This writing of Pascal was recommended for study to Leibniz by Huygens. [As given by Pascal in his letter to de Carcavi, containing the particulars of his method for centers of gravity and the definitions of "trilignes" and "onglets," the problems proposed in June were:

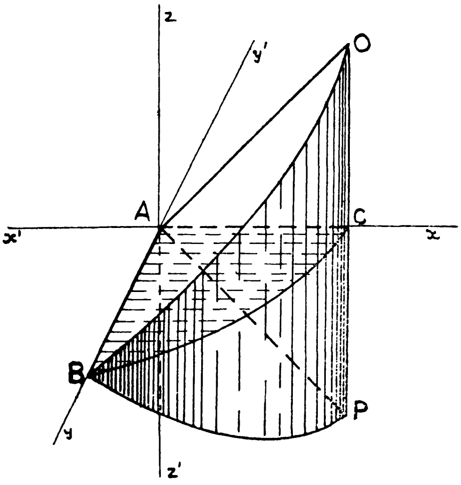


1. To find the dimension and the center of gravity of the space CYZ.
2. To find the dimension and the center of gravity of its semi-solid of rotation about the base ZY, i. e., the solid formed by the triligne CYZ when rotated about the base ZY through half a turn only.
3. To find the dimension and the center of gravity of the solid of revolution about the axis CZ.

To which are added the three proposed in the *Histoire de la Roulette* at the commencement of October:

1. To find the dimension and the center of gravity of the curved line CY.
2. To find the dimension and the center of gravity of the surface of the semi-solid about the base.
3. To find the dimension and the center of gravity of the surface of the semi-solid about the axis.]

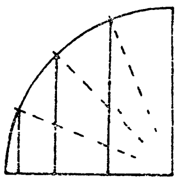
[17] By "Triligne" Pascal intends a plane figure bounded by two straight lines perpendicular to one another and a curved line. One of these perpendicular lines is called the axis and the other the base of the figure. If upon such a figure as a base there is erected a right solid, and this solid is cut by a plane which passes through the axis, or the base, then the portion of the solid that is cut off is called an "onglet." A "double onglet" is obtained if, through the solid formed by production on the other side of the base, there is



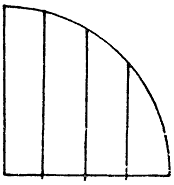
drawn a plane with the same inclination. [The last sentence does not make it clear that the second cutting plane also passes through the axis, or the base, as the case may be; nor that the plane is anticlinic and not parallel to the first plane; nor that Pascal took in general the inclination of the planes to the plane of the triligne to be 45°. I have therefore tried to represent the onglet and the double onglet in a diagram, see above.

ABC is the triligne, OABC is the onglet of (the axis or base) AB, and OBPCA is the double onglet of AB; the angles OAC, PAC are half right angles.]

[18] By *Sinus* Pascal intends the ordinates multiplied by the indefinitely small portions of the arc. [This is a very misleading statement; for Pascal distinctly distinguishes between *sines* and *ordinates,* and thus makes a considerable advance over his contemporaries. He defines them at the same time for finite section and for infinitesimal section; the distinction is made perfectly obvious in a diagram if we use finite section, say, division into four equal parts, of the quadrant of a circle as a special case of a triligne. Now the sum of the sines or the ordinates are defined as the sum of the rectangles (for, as with all cases of indivisibles, that is what it comes to), formed by the sines or the ordinates respectively multiplied by the corresponding *equal* sectional parts. Thus, to speak of the sum of the sines as being the ordinates multiplied by the small portions of the arcs is quite wrong. Though only in rare cases is the space drawn, Pascal's idea of the sum of the sines is that of the space formed by *straightening* the arc and erecting at each point of division the corresponding sine. Now, as Pascal says in Prop. 1 of the *Traitté des Trilignes,* the sum of the ordinates, which have to be applied to the base, makes the figure itself; while in Prop. 1 of the *Traitté des Sinus du quart de Cercle,* he shows that the sum of the sines (as a special case of the general theorem quoted in iii by Gerhardt *supra,* p. 534) of a quadrant is equal to the square on the radius. Thus, in modern notation,



SINES OF THE BASE.



ORDINATES OF THE BASE.

$$\text{sum of sines} = \int_0^{\frac{\pi}{2}} r \sin\theta \,.\, d(r\theta) = r^2$$

$$\text{sum of ords.} = \int_0^{\frac{\pi}{2}} r \sin\theta \,.\, d(r\cos\theta) = -\tfrac{1}{4}\pi r^2 \,.$$

The concluding paragraph of the *Traitté des Solides Circulaires* runs thus: "All these results arise from the fact that the straight lines OI are ordinates, that is to say that they are equally distant and proceed from equal divisions of the diameter; this brings it about that the simple sum of the ordinates is the same thing as the space intercepted between the extremes. But this is not true for the sines, since the distances between adjacent ones are not equal to one another, and thus the sum of the sines is not equal to the space intercepted between the extremes; *there must be no mistaken idea on this point.*" We find the same care taken by Barrow; but Tacquet breaks down in determining the surface of a cone through not understanding the necessity of this point, and in consequence condemns the method of indivisibles.]

[19] [The effect is as Gerhardt states, but these sums are differently ***defined*** by Pascal in his letter to de Carcavi. The triangular sum of the numbers or magnitudes A, B, C, D, starting with A, (which should be stated), is the sum of all of them, plus the sum of all of them except the first, A, plus the sum of all except the first two, A and B, and so on; this is represented by Pascal as in the margin, and he goes on to show that this is equal to the first taken once, the second twice, and so on. Thus defined, the reason why they are named triangular numbers is obvious. The pyramidal sum is similarly defined as the triangular sum of all, plus the triangular sum of all except the first, plus the triangular sum of all except the first two, and so on. As if there were built up a pyramid having the first triangular sum as its bottom layer, the second triangular sum as the next layer, and so on; thus defined, the origin of the name pyramidal is obvious. Pascal then shows that this is the sum of the quantities taken respectively once, three times, six times, and so on, according to the sequence of the triangular numbers. Then using the property that twice a triangular number diminished by its ordinal number is equal to the square of that ordinal (i. e., $n(n+1)-n=n^2$), he also shows that, if two such pyramidal sums of quantities are taken, and from one of them the bottom layer is removed (i. e., the first triangular sum), then the sum of the two is equal to the sum of the quantities respectively multiplied in succession by the squares of the natural numbers. There is no connection between this and what is usually known as the Arithmetical Triangle of Pascal (see *The Monist,* Oct., 1916, p. 603).]

$$\begin{array}{cccc} A & B & C & D \\ & B & C & D \\ & & C & D \\ & & & D \end{array}$$

$$\begin{array}{cccc} A & B & C & D \\ 1 & 2 & 3 & 4 \end{array}$$

[20] [Pascal simply states the results, as deduced, not from the theorem quoted by Gerhardt, but (together with the theorem quoted) from the preliminary lemma that the radius is to the sine as the hypotenuse of the infinitesimal triangle is to its base: in modern notation, $r:y=ds:dx$, or $r\,dx=y\,ds$, where y is a *sine* and not an *ordinate* in Pascal's sense. All the following theorems are particular cases of the formula $\int y^n\,ds=r.\int y^{n-1}dx$.]

[22] Descartes had spoken disparagingly about Pascal's "Essay on the Conics." Perhaps Pascal's decided opposition to Descartes may be traced back to this. Pascal's niece, Marguerite, writes: "M. Pascal used to speak very little about science; however, when the occasion for doing so occurred, he would state his opinion on those matters about which people were speaking to him. For example, with reference to the philosophy of Descartes, he merely said what he thought. He was of the same opinion as Descartes concerning automatism, but far from being so on the "subtle matter," which he ridiculed. But he could not put up with his (Descartes's) method of explaining the formation of the universe, and he often said: "I cannot pardon Descartes. In the whole of his philosophy, he would have been highly pleased to have dispensed with God; but he could not help making use of him to give a fillip to set the universe in motion. That being done, he had no further use for God." (Fougére, *Lettres, Opuscules et Mémoires de Madame Perier et de Jacqueline, soeurs de Pascal, et de Marguerite Perier, sa nièce.* Paris, 1845, p. 458). [It is more probable that Pascal used geometry, as Barrow did, because he both preferred it and thought it more rigorous than analysis. With regard to the remark on method, Gerhardt does not intend to convey the impression that Pascal abandoned for the more strictly geometrical method of moments the mechanical idea of the ***balance,*** with which he commences. By the way, to the best of my belief, the word "moment" is never used by Pascal.]

[22] [I have gone carefully through the "Lettres of Dettonville," and I find no mention of Archimedes except in one place, namely, Prop. 1 of the *Traitté des Solides Circulaires*; and the whole of this is devoted to ***volumes*** of solids and their centers. Nor can I find any place where Pascal determines the surface of a sphere, at least not by reducing it to an equivalent plane figure, I have however shown that Barrow does do this (see *The Monist,* Oct., 1916, pp. 610, 611). Surely Leibniz must be confusing the work of Pascal with that of Barrow on quadratures, the latter being so similar to the former in places that Barrow might easily be suspected of "borrowing" from Pascal; much more

easily indeed than Leibniz could be so suspected with regard to either, in spite of his own assertion with regard to Pascal. See Notes 23, 24.]

[23] [These are far more like Barrow's results than those of Pascal; while the style is entirely Barrovian and quite different from that of Pascal.]

[24] [There is no rectification of curves in Pascal; the whole of this sentence would however serve as a summary of the work of Barrow on rectification.]

[25] [Gerhardt states that the Centrobaryc Method, as considered by Leibniz in the manuscripts dated October 25, 26, 29, and November 1, 1675, shows clear connection with the work of Pascal. He asserts that, from a consideration of Archimedes, Pascal was enabled to extend the method of the ancients; he does not seem to be aware of what Cavalieri had done and published as the fifth section of his *Exercitationes Sex*; or else, knowing all about this, he suppresses that knowledge for fear of discrediting the statements of Leibniz concerning the methods of Cavalieri.

The striking points about the work of Cavalieri in question are as follows. He opens by defining gravity as a property of a body, a descensive force. He then defines a heavy body as one possessing this property, and in a note on the definition, he adds that these must be taken to include surfaces, lines, and points. Then he gives the definition of "moment" in its mechanical sense. "The moment of a weight is its endeavor to descend, no matter at what distance it is hung." This is followed by the note: "Since this moment is different at different distances, as will be seen in what follows, it is to be understood from this that the same weight may have different moments." He then defines uniform and uniformly variable (*difformis*) weights, such as a parallelogram in which the density varies as some power of the distance from one side; also he defines the centers of gravity and equilibrium. In Prop. 6 he shows that the moments of bodies are compounded of the ratio of their weights and the ratio of their distances. In Prop. 8 et seq., he combines the doctrine of indivisibles with that of moments to find the centers of gravity of surfaces, chiefly by means of "analogous figures"; thus, a uniform triangle is analogous to a parallelogram whose "difformity is of the first species," i. e., the density varies as the distance from one edge. He shows that, if the difformity is of the nth species, i. e., if the density varies as the nth power of the distance from the edge, then the medial line is divided by the center of gravity into parts in the ratio of 1 to $n + 1$, although it is stated rather differently, and only worked out for the first few values; then, using the idea of moments he proceeds from one degree to another in the case of the triangle, where the axis of moments (*limes*) is a parallel to the base through the vertex, and in the following proposition, the base itself; next the semicircle and the hemisphere are dealt with, whether uniform or varying as the distance from the center. In Prop. 36, he lays down the idea that the axis of moments may be outside the figure under consideration; and then proceeds to consider cylinders, cones, parabolic conoids, and the sphere, and truncated portions of them; and finally he finds the moment of a portion of a hyperbola about the asymptote which is not the base of the portion considered. It is interesting to note that Cavalieri, when speaking of the difformity of weight, uses the phrase "***incrementum*** difforme gravitatis," i. e., the word ***incrementum*** is employed to connote a *gradual* increase that follows a definite law. Also it is worthy of remark that he employs the notation, *o. l., o. p., o. q., o. c.,* etc. for "all the lines," "all the planes," "all the squares," "all the cubes," etc.

From the above it will be seen that Cavalieri has given a fairly comprehensive account of the use of moments for the determination of the center of gravity; thus he not only gives far more than Pascal, but anticipates him. Leibniz's matter is far more like that of Cavalieri than that of Pascal; though he seems to be reading Pascal at the time he wrote the third part of the "Analytical Quadrature," by the method of moments, for the last figure in this manuscript (see *The Monist,* April, 1917, p. 268), with the explanatory diagram that I have added on the right of it, is strongly reminiscent of the idea of the onglet of Pascal; although it may have arisen from Cavalieri's work. The great point about this batch of manuscripts of October and November, 1675, is that nearly every figure has the tangent drawn to the curve; now the tangents are never drawn or used either by Cavalieri or by Pascal. A secondary consideration, but still one of importance, is that the subject-matter of these manuscripts is like nothing in Cavalieri or Pascal, as far as the "center of gravity method" is concerned. As we find Pascal's Infinitesimal Triangle idea in the figure of Leibniz's manuscript of October, 1674, I take it that this was the time at which he *finished reading* his Pascal. Hence, I imagine that in October, 1675, he had got a good knowledge of Descartes's algebraical geometry, and began to study Cavalieri's *Exercitationes Sex*; he did not get very far in this before he appreciated the power given by the method of moments; then, probably wearied by Cavalieri's prolix demonstrations, he laid the book aside, and applied Cartesian analysis to the method of moments, running the idea for all it was worth. If this is the case, these manuscripts represent real *original* research, and are not study notes like some of the others.]

[26] [The misreadings of Gerhardt, as given in his *Geschichte der höheren Analysis* (see *The Monist,* April, 1917, p. 244) are uncorrected even in 1891, the date of this essay, thirty-six years after the publication of the *Geschichte*! We should have "$\mathrm{AB} \sqcap \mathrm{DC} \sqcap x$" and "$\mathrm{AB}(=x)$"—see the figure on the right (p. 538), which is mine while that on the left is the one that Gerhardt gives as that of Leibniz; again Gerhardt's "id est *ac* in omn.x, sive $a(cb^2/2)$," which makes Leibniz write nonsense, should be "id est a c in omn.x, sive a $cb^2/2$," the "a" being the preposition "away from" and not the length of a line; thus corrected we not only have a sensible reading but the whole paragraph is correct; I have made the correction when translating. Also with regard to Gerhardt's statement that Leibniz starts from an alternative rendering of Prop. 2 of Pascal's *Traitté des Trilignes,* it is worthy of remark that Pascal's figure is altogether different from that of Leibniz; and this is only natural, because there is *no similarity between the theorems, nor is there any relation between the methods of proof.* Pascal's proof is equivalent to the modern method of a change in the independent variable by a conversion to a double integral followed by a change in the order of integration, and is geometrical; that of Leibniz is equivalent to integration by parts, and is merely an example of the theorem of moments.

Thus (Pascal), $\int yx\,dx = \int(\int x\,dx)dy = \int \tfrac{1}{2}x^2dy,$
and (Leibniz), $\int yx\,dx = [\tfrac{1}{2}x^2y] - \int \tfrac{1}{2}x^2dy;$

where Pascal's integrals are taken over the *same* area as one another, and those of Leibniz over *complementary* areas. It seems therefore ridiculous to say that "Leibniz commences with Prop. 2.... which he expresses as follows."]

[27] [This means the result obtained geometrically by means of the triangle AZC, in the passage to which Note 23 refers.]

[28] [The connection between number, ratio, and *infinitesimal* is peculiar.]

[29] [Note the word "useful" (*utile*): the "long s" is introduced merely as a convenient abbreviation in accordance with Leibniz's usual idea of obtaining simplification by means of symbols.]

[30] [I have discussed this fully in my translation of Gerhardt's essay, "Leibniz in London" (see *The Monist,* Oct., 1917, p. 545). I have shown there that at least it is highly probable that the d in x/d stands for a certain length, namely the subtangent.]

[31] [Note that, in spite of Gerhardt's opening remarks about the algorithm of the calculus being due to reading Pascal, the symbols of integration and differentiation have not been mentioned in anything quoted by Gerhardt in this essay, except in the paragraph just above.]

[32] [See *The Monist,* Oct., 1917, where a translation has been given. I believe some of those who read what is there given will, while giving Leibniz full credit for the introduction and *development* of the symbols $\int$ and d, that made the calculus of Leibniz the powerful instrument it was, will find it hard

if not impossible to agree with Gerhardt in his assertion that the ideas of Leibniz were not very strongly influenced by the best points of every single author that he studied, and more especially by the *Lectiones Geometricae* of Barrow and the *Exercitationes Sex* of Cavalieri.]

B. Notes to the Manuscripts of Leibniz.

I.

[33] [So far I have failed to find any information as to the error into which Reginaldus fell; he does not appear to be mentioned by either Cantor or Zeuthen.]

[34] [The Geometry of Cavalieri is indeed practically all quadratures; but Torricelli himself says (quoted by Tommaso Bonaventura in his preface to an edition of the *Lezione Accademiche,* 1715), in his preface to a *Tract on Proportion,* that he has used indivisibles for tangents as well as for quadratures; Roberval, through his own efforts at concealing his methods, we know comparatively little about; but the germ of Fermat's method is the same as that of Barrow's, namely the Differential Triangle; lastly it is probable that Huygens's knowledge was considerably more than he let anybody know (and so too with Gregory)—cf. Leibniz's words, "suppressing their analysis," a few lines later. It is to be observed that Leibniz deliberately speaks of the mathematicians of France and Italy only; "at the present time," 1679, he must have been aware that Barrow had complete geometrical knowledge, at any rate, of *all* the matters in question.]

[35] [The *Horologium* was published in March or April, 1673, and the presentation of a copy to Leibniz was undoubtedly made *after* his return from his first visit to London (Cantor says that the dedication was dated March 25, 1673; see Cantor, III, p. 138). Hence, the date at which Leibniz obtained the Characteristic Triangle can be assigned to some time at least not later than the beginning of October, 1673; and therefore the inclusion of this in the manuscript dated Aug., 1673 (see *The Monist,* April, 1917, p. 238), marks the exact date of its discovery.]

[36] [In the "Bernoulli postscript" (see *The Monist,* Oct., 1916, p. 584), Leibniz states that "he obtained a Dettonville from Buotius, a Gregory St. Vincent from the Royal Library, and started to study geometry in earnest." In the *Historia* (see *The Monist,* Oct., 1916, p. 595) Leibniz says that, "in order to obtain an insight into the geometry of quadratures he consulted the *Synopsis Geometriae* of Honoratus Fabri, Gregory St. Vincent, and a little book by Dettonville (Pascal)." In his letter to the Marquis de l'Hospital he says, "At the start I only knew the indivisibles of Cavalieri, and the 'ductions' of Father Gregory St. Vincent, along with the 'Synopsis of Geometry' of Father Fabri" (see *supra,* p. 544). I suggest that the correct explanation of these inconsistencies is that he did get the Dettonville from Huygens as stated here, the St. Vincent from the Royal Library, and the work that he obtained from Buotius was the *Exercitationes Sex* of Cavalieri.]

[37] [I think the passage throws considerable light on the character of these manuscripts, besides explaining how it was that Leibniz seems to have taken a very long time to study the works of the authors mentioned. I look on these manuscripts, not as "study notes" merely, nor yet as true "research," but as a mixture of each. I suggest that there is quite enough evidence to make it safe to assert that the characteristic of Leibniz's method of study was to read a very small portion of an author at a time, then to break off and follow out the train of ideas suggested to him by the passage to the furthest limit, before proceeding further with his reading; thus he is led to his own *original* developments. For instance, note in the next sentence how he says he "tried to find a *new* sort of center." This is very characteristic; he is not satisfied with merely acquiring knowledge, even at this early stage, but at once seeks to utilize each point, *as he grasps it,* to obtain something new, something

original previously undiscovered. Cf. the study notes on the work of Pascal, given above under III.]

[38] [That is, a "homothetic center."]

[39] [As I have been unable to find the word "moment" defined, or even mentioned, in any place except in the *Exercitationes Sex* of Cavalieri, I suggest that this is fairly good circumstantial evidence for the reading of this work by Leibniz before he discovered the theorem in question.]

[40] [Observe that this is not the figure used in the manuscript of October, 1674 (see *The Monist,* April, 1917, p. 241), the latter being a diagram that one would naturally expect him to have obtained from the figure in the lemma that commences Pascal's *Traitté des Sinus du quart de Cercle* (cf. Note 6) ; but is a figure such as one would expect Leibniz to abstract from those given by Barrow, either from Lect. XII, prop. 1, 2, 3, or from Lect. XI, Prop. 1 (see *The Monist,* Oct., 1916, p. 610 and p. 616 respectively. In the latter especially we have the right-angled triangle used by Leibniz on page 596, quoted by Gerhardt in the article translated in the present number). I therefore suggest that Leibniz worked at Barrow and Pascal conjointly, and applied Descartes's analysis to their geometrical theorems. If this is not the case, Leibniz was at fault, for Pascal was discussing *sines* and not *ordinates* (see Note 18) ; i. e., Pascal was integrating with regard to θ and not with regard to x. Observe also that the figure as given is not correct; the rectangle should be that having AC, CD as adjacent sides.]

[41] [Note that the area is taken to be produced by the assemblage of lines applied in order, in the true Cavalierian style.]

[42] [Query: urged thereto by a question on the part of Huygens, as to whether Leibniz could now find the properties of the auxiliary curve (see *The Monist,* Oct., 1916, "Bernoulli postscript," p. 585).]

[43] [This fits in perfectly with my suggestion that Leibniz attacked Barrow's *Lectiones* at several different times. Having, as I think, taken Barrow's advice given in the preface, he sampled the first few propositions of each lecture, and obtained from those of Lect. XI and XII his Characteristic Triangle. This could I think have been definitely settled if Gerhardt had only given the figure used by Leibniz in the manuscript dated August, 1673. Assuming for the time being that my suggestion is correct and that Leibniz is merely confusing the author that he read at this time, I suggest that characteristically he broke off his reading of Barrow, pursued the idea he had obtained, and made out those theorems on quadratures that he speaks of; this so improved his geometry that later he was able to read Barrow thoroughly and appreciate all that was in it, and to find that his theorems had been anticipated. I also suggest that it was on this second or third reading that he came across the theorem that led to his Arithmetical Tetragonism. A fresh reference to Barrow to find if there were any other ideas that he could develop, considerably later, having already found him a mine of information, would then probably be the occasion on which the marginal notes in his own notation were inserted by Leibniz.]

[44] [Leibniz seems to have got these men in true perspective, Cavalieri, Fermat, Gregory, and Barrow, as far as the infinitesimal calculus is concerned. But I doubt whether he, even after he came to his fullest appreciation of Barrow's *geometrical theorems,* or indeed any other person except Bernoulli, ever appreciated the real inwardness of these theorems, or that Barrow's tangent problems could be used, in the manner I have shown in the appendix to my Barrow, to draw a tangent to any curve *given by an equation in either Cartesian or polar coordinates.*]

[45] [This I take to mean the principle that differentiation and integration are inverse operations; for it is practically certain that in November, 1675, he could not differentiate a product; otherwise, as previously argued, he would

have verified his solution of the unfortunate equation, $x + y^2/2d = a^2/y$, which he gives as

$$(y^2 + x^2)(a^2 - yx) = 2y^2 \overline{\text{Log}\, y},$$

by differentiation, as he did with a previous solution that did not contain a product.]

[46] [From this probably arose the first germ of the idea of the Quadratrix, in the sense used by Leibniz.]

[47] [Substitute *Barrow* and Mercator in conjunction, and we have a feasible suggestion for explaining the first method of proof for the Arithmetical Quadrature of the Circle; the method that Leibniz does not seem ever to have divulged.]

[48] [It is impossible for me to conjecture exactly which of his ideas is here referred to by Leibniz; for he calls a mere method by the name of "a calculus," and what we should call a dodge for some particular kind of example by the name of "a method." I think it may be possible that the "transmutation of figures" is referred to.]

II.

[49] [Notice that Leibniz says that he has not derived any help from Barrow for his ***methods*** (*je n'ay tiré aucun secours pour mes methodes*). This is less even than he might have said with perfect truth; for the ***methods*** of Barrow would have been a veritable hindrance to Leibniz's analytical development. Even when using the Differential Triangle method, and literals for the lengths of his lines, the whole of the working is geometrical in the examples of the method given by Barrow, and not analytical.]

[50] [See Notes 35, 36.]

[51] [Perhaps this is meant to include Barrow.]

[52] [Notice the words "by chance" (*par hasard*); these seem to point to a conclusion that Leibniz read the Pascal in a very desultory manner; this conclusion gets corroborated by the extract given by Gerhardt under the heading III. It is worthy of remark that the "by chance," or "incidentally" (as I have rendered Leibniz's word *forte* in the letter to Tschirnhaus), is made to refer to Pascal. *"Forte Pascalius demonstratbat,"* etc., i. e., "Incidentally Pascal was proving," etc. I think it may be asserted that Pascal missed absolutely nothing that was pertinent to *his purpose*; whereas Barrow certainly missed the opportunity of being the discoverer of the series for the inverse tangent, and thereby the quadrature of the circle, by not applying Mercator's method of division and integration to the result of one of his examples of the Differential Triangle method; as also after giving the method of "transmutation of figures" he missed those things to which it led.]

[53] [In a manuscript dated October, 1674 (see *The Monist,* April, 1917, p. 240), Leibniz is using x and y for the variable ordinate and abscissa; while in a manuscript dated August, 1673, he considers "the classification of curves laid down by Descartes." In this manuscript, according to Gerhardt, Leibniz has already constructed the "characteristic triangle," but Gerhardt does not give the particular variant that Leibniz uses in this manuscript. I believe that this will prove to be of the Barrow type, when reference can be made to the original; for the title of the manuscript is strongly suggestive of Barrow, being: *Methodus nova investigandi Tangentes....ex datis applicatis,* etc.; and Pascal's work does not mention tangents.]

[54] [That is, as the Characteristic Triangle, leading to integrations, is ascribed to the influence of the work of Pascal, so the Differential Calculus is ascribed to the influence of the work of Descartes. Is this the diplomatic characteristic in Leibniz peeping out? He is writing to a Frenchman, and attributes his work to the respective influences of two Frenchmen. Note that

Leibniz goes on to state that the source of inspiration was summation of series by differences, suggesting the origin of the symbol *dx*.]

[55] [In the manuscripts that we have had under consideration, Leibniz does not appear to have made any practical use of the Quadratrix.]

[56] [It is precisely this point which formed the really great improvement in the reckoning section of the infinitesimal calculus. It is just this improvement that is due to Leibniz in analysis, and to Barrow in geometry; although Leibniz did not accomplish anything of the kind until 1676 or 1677. Newton's method by means of series for fractions and roots does not bear comparison, let alone the futility of ascribing Leibniz's method to a perusal of Newton's work.]

III.

[57] [All that is any good in the following is to be found in Pascal; I think this corroborates the suggestion I have made as to Leibniz's way when studying a book. It looks here as if he had read about twenty pages of Pascal, and about the same number of pages of Cavalieri's section on centers of gravity; moved thereto probably or possibly by Pascal's remark "....the principle of *indivisibles,* which cannot be rejected by any one having pretensions to rank as a geometer." Then he proceeds to work out his own combination of the two ideas, without bothering to see what else either of these authors had to say on the matter.]

[58] [Leibniz tacitly assumes that all the points are occupied; this is necessary for the success of the notion of triangular sums.]

[59] [Something very like this is indeed investigated fairly thoroughly in a manuscript dated October 25, 1675 (see *The Monist,* April, 1917, p. 245). Hence these extracts from Pascal were certainly made before that time, though probably not long before.]

[60] [This is the rendering for "*productum fieri aequale*"; he probably means that what is produced on the one side, i. e., the sum of the moments on one side of A, should be equal to the sum of the moments on the other side. But this endeavor to obtain *something new* seems rather futile.]

[61] [It would have been interesting to have seen what this simple rule was. Probably nothing more than the propositions given by Pascal as Prop. 1, 2, 3 of his method of the *balance*; this would corroborate my suggestion that Leibniz did not study Pascal very steadily or thoroughly (cf. Notes 37, 43, 52, 57).]

SUMMING UP.

The notes and criticisms that I have made in these five articles on the manuscripts of Leibniz may give the impression that I am an anti-Leibnizian. This is quite wrong. My prime object was to show, to the best of my power, that the charges of plagiarism brought against Leibniz by partisans of Newton, and indeed by Newton himself in the *Recensio* published in the *Philosophical Transactions,* were unfounded. I considered that the charges in the *Recensio* were perhaps the hardest to be answered, since they were not only direct charges, backed with circumstantial evidence, but they were also set forth very cleverly. Also I thought that the method of defense adopted by Gerhardt and other partisans of Leibniz did as much harm to him as the strongest attack of avowed opponents, such as Sloman. If I am anti-anyone, I am anti-Gerhardt. Never surely did any man have such a glorious opportunity as Gerhardt, in the whole history of scientific controversies; surely there never was an advocate who left himself so open to the attacks of the opponents. Gerhardt starts with the theory that every single word of Leibniz represents gospel truth; and that it is almost blasphemy to doubt it; in consequence he is soon in difficulties when it comes to reconciling the varying statements of the sequences of events that are made by Leibniz at different times. But, once the idea is accepted that Leibniz, while perfectly reliable on the general run of events, is unreliable when it comes to unimportant details, and then all difficulty disappears. I therefore set out with the determination to break down, if I could, the credibility of Leibniz as a witness in his own defense, when it came to unimportant details; then to show that he had opportunities for obtaining everything necessary to the development of the Calculus, that he could not be expected to supply for himself by original work, without having need to know anything of the work of Newton; then to show that these sources of information were set out in a form far more suitable to the requirements of Leibniz than the work of Newton; finally, to clinch the matter, that the analogy of Leibniz's work was so close to these sources, that it was idle to suppose that he made use of any other sources. In other words, (i) the *Analysis per aequationes* was unnecessary to Leibniz, (ii) Newton's method of dodging fractions and roots by means of infinite series was clever, but futile for the needs of Leibniz when developing an operational calculus.

The unreliability of Leibniz with regard to details may be in some measure due to his admittedly bad memory (which is evidenced by his habit of committing everything to writing), and to passage of time. But in a far greater degree it must be ascribed to the circumstances and characteristics of Leibniz. We know that he designed to compile an encyclopedia of *all* science, and for this he considered not at all the nationality or the personality of the discoverer or the author: all he was interested in were the facts or principles discovered.

That he was unreliable with regard to details is proved by the facts I have adduced:

i. the confusion between Mouton and Mercator in the account of the first charge of plagiarism made against him, or rather an assertion that he had been anticipated (see *The Monist,* Oct., 1916, p. 594, and Note 73);

ii. the varied assortment of figures that he gives to illustrate how he found the Characteristic Triangle (see *The Monist,* Oct., 1916, p. 585, and compare them with the figures given in the accounts quoted by Gerhardt in this essay and those published in *The Monist,* Oct., 1917);

iii. the circumstantial detail of the context of the Archimedean measurement of the surface of the sphere being absent from the author he quotes;

iv. the several different accounts of the order in which he obtained his different books for study, and even the persons from whom he obtained them;

v. the error with regard to the time of the presentation of the copy of the *Horologium* (see *The Monist,* Oct., 1916, p. 594, where, in the *Historia,* it is stated that he received it *before* he left for England on his first visit);

vi. the confusion as to the date at which he obtained his Barrow (see *The Monist,* Oct., 1916, p. 586, where, in the Bernoulli postscript, he states that he found the greater part of his theorems anticipated in "Barrow, when his *Lectures* appeared");

and many other things, all unimportant details singly; but, when taken in combination, they show distinctly that we must only take Leibniz's word as accurately describing the *general* course of events.

Another characteristic of Leibniz seems to have been insistent at all times; he burned to distinguish himself as a discoverer of new things. I have suggested that there may have been an ulterior motive to this desire, namely, to get himself taken into the select circle of mathematicians who corresponded with one another. Thus, when he studied an author, and came across some new idea, he would break off his reading to follow that idea to the limit and exhaust all its possibilities, committing his results to writing, whether they were important or not; there is some evidence, too, that while doing this, he would refer to other authors who had discussed the point under consideration, before returning to his reading.

My motive in trying to show that he got everything from Barrow, *except his methods,* was to remove any charge of plagiarism; for, I consider that even if he had merely rewritten Barrow in terms of Descartes, adding his own notation for the sake of convenience, he would still have done a great thing, and would no more have been guilty of plagiarism from either Descartes or Barrow than Stephenson was from Watt, or Parsons from either of these. Leibniz's Calculus was his own, and would have been his own even on the supposition above. Lastly, it was not only more complete than that of Newton, in that it was an operational calculus, though it did perhaps miss the idea of rate; but also from an intellectual standpoint it was greater, in that it was developed, after its first principles were found out, as a practical theory, while Newton's was developed as a mere instrument for his own purposes.

Assuming, then, that Leibniz did not remember, or did not really care, what his text-books were, so long as he was not accused of using somebody else's *methods,* I will try and reconstruct the progress of his reading and his discoveries. His text-books were,

i. Lanzius and Clavius in algebra, and Leotaud for geometry, in his early youth; he also looked through, more or less without understanding them, Descartes and Cavalieri's *Geometria Indivisibilibus.*

ii. On his return from London he brought back with him Barrow, some portions of which he had glanced at in London and on his journey; he obtained Pascal, St. Vincent, and Cavalieri's *Exercitationes Sex,* perhaps a little later than the others; besides these, Wallis and Mercator specially.

He read portions of the Barrow afresh, and obtained the Characteristic Triangle, and found his general theorem from this; meanwhile he is also studying Descartes, and we have the materials for the manuscript of August, 1673. Probably he has had a look through Pascal during this time. He remembers the similarity between the complicated diagrams of Barrow and some of those of Pascal, and starts studying the *Traitté des Sinus,* in which he finds the second variant of the differential triangle that appears in the manuscript of October, 1674. Previous to this, however, his attention has been arrested by Barrow's proof of the inverse nature of the operations of finding a tangent and an area, and the analogy between this and sums and differences strikes him. He has also considered the examples on the differential triangle given by Barrow; one of them suggests the method of Mercator to him, he has already got an idea from Wallis of the summation of the several powers of the variable; he applies this to Barrow's expression, equivalent to

$$d(\tan^{-1}x)/dx = 1/(1+x^2),$$

in modern notation, performs the division as Mercator had done, and obtains the series for the inverse-tangent by a summation according to Wallis, i. e., practically an integration. This answers the charge made by Newton that somehow or other he got this series from him or James Gregory. In the same way, he thought that he could obtain other series, but later found that it was beyond his power. We find in this manuscript of October, 1674, an attempt to get something out of an analogous series, the logarithmic series, showing that it is very probable that he has been studying Mercator during the interval between August, 1673, and October, 1674. And in the *Historia* he definitely states that he came upon the Arithmetical Tetragonism in 1674; so that I think that I have offered a reasonable suggestion as to the course his studies took so far. Also in the meanwhile he has been doing a lot of work on series, and has invented his Harmonic Triangle. I now suppose that he completes his study of Pascal, is led by a remark in it to study the *Exercitationes Sex* of Cavalieri (he has already got some acquaintance with the *Geometria Indivisibilibus*, read as a youth), he does not find much in that to his liking, except the notion of moments. He breaks off his reading and proceeds to work out an application of Descartes's algebra to this new idea of moments, the result being the manuscripts of October and November, 1675; here he is led on to the introduction of the symbols for summation and differentiation, though as yet applied to series, and sums of powers. The consideration of the Quadratrix leads him to make a further study of Barrow; and he is led to x/d, by a consideration of Barrow's propositions on the inverse nature of the operations of integration and differentiation. This, combined with the analogy to the inverse nature of summations and differences, leads him to search for a reason why x/d should represent a difference such as he has considered to be denoted by dx. This at a later date necessitates the discussion of what the result of *operating* with *d* on a product or a quotient will be. Meanwhile the study of Barrow brings him to that proposition which gives the polar differential triangle; in it he perceives at once the method of "transmutation of figures." I now suppose that he appreciates Barrow more fully and begins to apply Cartesian geometry to Barrow's theorems; in a manuscript dated November, 1675, he attacked the problem of tangents, and in connection with it considered the method of Descartes. In the next manuscript that we have, dated June, 1676, he practically obtained the differentiation of the sine and the inverse sine; his figure, if he had given one, would have been the same as that of Barrow for the differentiation of the tangent. In July, 1676, he attacked the inverse-tangent problem, still considering the work of Descartes. I think, however, that his work on Barrow has taken effect, for from now on he includes the differential factor *dx* under the integral sign. This is the last manuscript before he went to London for the second time.

Thus, I take it that all Leibniz's work is the result of his own original methods on ideas that have been suggested chiefly by two books, those of Barrow and Descartes; at least, everything could have been suggested by these two books alone, except the notion of "moment," which came from Cavalieri. Thus it was unnecessary for him to have known anything about the work of Newton before he went to London for the second time. What he saw there may have had the effect of corroborating his own work; it could have had little other effect. The final polishing of his method I put down to a study of the Differential Triangle method of Barrow, which Leibniz perceived to be powerful, but found distasteful on account of the geometrical nature of the work.

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